« Do European PES help reduce herbicide use? Evidence from a natural experiment in France»
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Abstract

Payments for Environmental Services (PES) are a central component of the environmental policy of the European Union, but few of these programs have been carefully evaluated and doubts are often expressed about the effectiveness of voluntary programs. We use original data collected from winegrowers participating in PES targeting nonpoint source pollution from pesticides. We use exogenous variation in the timing of the implementation of the program as a natural experiment. We show that the quantity of herbicides used by participants in the program in 2012 was around 30 percent below what they would have used without the program, while the impact was significantly higher in 2011 - around 50 percent - presumably because of higher weed pressure. We moreover estimate a windfall effect associated with the least stringent measure.

Keywords: Payments for Environmental Services, agri-environmental scheme, water quality, nonpoint source pollution, herbicides, pesticides, natural experiment, treatment effect.
JEL: Q15, Q18, Q25, Q28, Q53.

1 Introduction

In the mid-1980s, increasing concern over the environmental impact of agriculture led to the introduction of agri-environmental schemes (AESs), also labeled as Payments for Environmental Services (PES) in recent years, in the European Community. AESs are voluntary
contracts under which farmers are offered payments for reducing the negative externali-

ties of agricultural production, and constitute a central component of the environmental

policy of the European Union. However, doubts are often expressed about the effective-

ness of these programs. Because AESs are voluntary programs whose requirements and

per-hectare payments are generally constant for all farmers, the potential for adverse se-

lection is high. Indeed, farmers receive payments in exchange for adopting certain agricultural practices, ones that they may well

have adopted even in the absence of these payments. In the extreme case in which an AES

attracts only farmers who would have behaved the same way in the absence of payment,

the additional effect of the AES is null, while the windfall effect is maximum.

Despite widespread interest and investment in AESs (Uthes and Matzdorf, 2013; Uda-

gawa, Hodge and Reader, 2014), few programs have been thoroughly evaluated. Chabé-

Ferret and Subervie (2013) show that French AESs that impose strong requirements, such

as the AES subsidizing conversion to organic farming, had large additional effects and al-

most non-existent windfall effects. On the contrary, the authors find that AESs with mod-

est aims, such as the AES only requiring farmers to add one crop to the rotation, have

generated very limited additional effects. Pufahl and Weiss (2009) moreover show, from a

non-representative sample of German farms, that benefiting from AESs may significantly

reduce the purchase of farm chemicals. To our knowledge, there has been no evaluation

of AESs specifically targeting the use of pesticides to date.

Contamination by pesticides from agriculture is a source of water quality degrada-

tion in several countries in the European Union. This occurs when pesticides used in

fields are picked up and carried away by runoff and deposited into lakes, rivers, wetlands,

coastal waters, and underground sources of drinking water. These pollutants are of in-

creasing concern because of their potential impacts on the environment, wildlife, and hu-

man health. Within the context of the European Union water framework directive, French

\[\text{\footnotesize{The European Agricultural Fund for Rural Development (EAFRD) has been allocated a budget of EUR 96.3 billion for the period 2007-2013 (20% of the funds dedicated to the CAP), of which EUR 1.8 billion has been allocated to French AESs. Figures are available here: http://agriculture.gouv.fr/pac-developpement-rural-lead.}}\]
AESs that aim to reduce pesticide runoff from fields have been implemented in watersheds where water quality improvement has been identified as a priority. Despite their importance, these AESs have received little attention with respect to their impacts on agricultural practices. The purpose of this article is to estimate the additional and windfall effects of AESs targeting nonpoint source pollution from pesticides, focusing on one emblematic case study: herbicide use in French vineyards.

Of all the cropping systems in France, wine growing uses the most pesticides, with an average application of 16 phytosanitary treatments per hectare in 2010. Indeed, growing wine grapes requires high levels of protection against bio-aggressors and competitive weeds in order to ensure adequate levels of production. Among the chemicals used by winegrowers, herbicides are the most commonly detected in the ground and surface waters. Given the extent of winegrowing and its heavily reliance on herbicides, incentivizing winegrowers to reduce their use of herbicides is a major challenge. Languedoc-Roussillon contains the most vineyards of any region in France, covering 236,500 ha and constituting 30% of the nation’s vineyards. Two out of every three farms in the region grow wine grapes. The AESs implemented here are area-specific and designed to address specific environmental issues, including water pollution. These AESs are 5-year contracts. Farmers apply voluntarily. The payments offered to participants are calculated to compensate for any lost income and for any additional associated costs.

AESs targeting pesticide use were introduced in 2007 and include a major innovation compared to previous AESs: they target the most environmentally-sensitive sites and are implemented by local operators (hereafter referred to as site operators). These AESs are currently implemented in 29 watersheds in the Languedoc-Roussillon region, most of which exhibit levels of herbicide residues exceeding the regulatory limit.

The main alternatives to using herbicides for weed control are mechanical methods such as tillage, controlled grassing, and mowing. However, mechanized weeding under

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2 In practice, the income forgone is based on a national price scale. It is calculated using national average values of additional expenses incurred through increased working hours and equipment hire for alternative practices to chemical weeding, taking into account reduced spending on herbicides. The payments also cover the costs of annual on-farm follow-up visits by an agreed technician, on the basis of the price scale applied by technicians in France.
vine rows requires specific investments, and farmers willing to reduce their use of herbicides without these further investments usually combine the use of mechanical alternatives between the rows with chemical weeding underneath the rows. Because these alternative techniques have been increasingly implemented by farmers (19 percent of vineyards using no herbicides before the AESs were launched (Agrete, 2012)), the additional impact of the monetary incentives offered to farmers for reducing their use of herbicides deserves evaluation.

Previous work on the evaluation of AESs relies on non-experimental data and has relied on identification strategies that address the issue of self-selection bias (Chabé-Ferret et Subervie, 2013; Pufahl et al., 2009; Udagawa et al., 2014). To do so, these studies employed DID-matching methods, which eliminate selection bias by comparing participants in AESs to observationally-identical non-participants, assuming that farmers’ self-selection into AESs is due to both observable and unobservable factors that are constant through time (Heckman, Ichimura, and Todd, 1997). We depart from the approach used in these studies by exploiting exogenous variation in the timing of the implementation of the AESs. Because the process governing the eligibility of winegrowers for an AES arguably resembles random assignment, we are able to circumvent the empirical issue of self-selection and to estimate the causal effect of the AES on pesticides use. We are moreover able to assess this effect over time because our dataset includes almost all participants in the AES under study in two consecutive years: 2011 and 2012. We show that the quantity of herbicides used by participants in the program in 2012 was around 30% below what they would have used without the program, while this impact was significantly higher in 2011 - around 50% - presumably because of higher weed pressure. Our results moreover show that the farmers who engaged in the “zero herbicide between the rows” option, both the most often chosen as well as the least stringent measure, would not have applied as much herbicides as expected in the absence of the AES, which indicates the presence of a windfall effect. Although significant overall, the specific impacts we find remain smaller than those initially expected by policy makers. Simple extrapolation of these results moreover suggests that these AESs may not be sufficient to ensure improved water quality in the watersheds.
targeted by the AES.

The remainder of this article is organized as follows. We first present the identification strategy, then the data used. Thereafter we present and discuss the results of the evaluation of the overall program and of one specific measure - the so-called “zero herbicides between the vine rows”. We present results that are based on the natural experiment assumption, as well as those we obtain from various matching estimators. We also present the results of several robustness checks and of a sensitivity test. Finally, we present some illustrative figures in order to discuss the likely impact of the AES on water quality.

2 Identification Strategy

2.1 Parameter of interest

The parameter of interest that we aim to estimate is the average causal effect of the AES on the amount of herbicides used by those who actually participated in the AES, the so-called Average Treatment effect on the Treated (ATT):

\[
ATT = E[Y^1 - Y^0 | AES = 1] = E(Y^1 | AES = 1) - E(Y^0 | AES = 1)
\]

The average level of herbicide that would have been used by participants, had they not participated in the AES, \(E(Y^0 | AES = 1)\), is unobservable to us. This is the standard problem of causal inference. The purpose of the empirical analysis is precisely to estimate this level.

The main concerns when evaluating the impact of AESs relate to the fact that participants self-select into the program. Because there may be systematic differences between participants and non-participants, most work on the evaluation of AESs employ matching methods, which eliminate selection bias by comparing participants in AESs to observationally-identical non-participants (Chabé-Ferret and Subervie 2013; Pufahl and Weiss 2009; Udagawa, Hodge, and Reader 2014). We depart from the approach used in these studies by exploiting exogenous variation in the timing of the implementation of the AESs. Under some conditions, this identification strategy allows us to use future-
participants in the AESs as controls. Take the example of the AES impact in 2011, which is defined as follows:

\[
\text{ATT}_{2011} = E(TFI_{12011}^1 | AES_{2011} = 1) - E(TFI_{02011}^0 | AES_{2011} = 1)
\]

where \( E(TFI_{12011}^1 | AES_{2011} = 1) \) measures the amount of herbicides actually used in 2011 by those who were participants in the AES in 2011 and \( E(TFI_{02011}^0 | AES_{2011} = 1) \) measures the amount of herbicides that would have been used in 2011 by these winegrowers, had they not participated in the AES. From the group of winegrowers who entered the AES in 2010, we are able to estimate \( E(TFI_{12011}^1 | AES_{2011} = 1) \) directly using the mean value of the TFI. Obviously, we are not able to estimate \( E(TFI_{02011}^0 | AES_{2011} = 1) \) from the same group. However, we can estimate this from the group of individuals who entered the scheme in 2012, provided the following assumption holds:

\[
E(TFI_{02011}^0 | Z, AES_{2011} = 1) = E(TFI_{02011}^0 | Z, AES_{2012} = 1)
\]

where \( Z \) denotes the set winegrower’s characteristics, observable or non-observable, driving his decision to participate in the AES. This assumption means that the mean value of the TFI that we observe in 2011 among those who entered the AES in 2012 equals the mean value of the TFI that we would have observed in 2011 among those who entered the AES in 2010, had they not participated in the AES.

### 2.2 Exogeneity assumption

In order to discuss to what extent such an assumption is likely to hold, we must examine a farmer’s decision to enter the AES. We model the winegrowers’ decision to enter the AES in the following way:

\[
AES = 1 \{ E(V_1 - V_0 | Z) - W \geq 0 \}
\]

where \( W \) is the disutility of applying for the AES (due to various transaction costs, for example), \( V_1 \) is the indirect utility of the individual when he participates, and \( V_0 \) is the indi-
rect utility when he does not. The selection bias problem occurs when some factors stored in $Z$ are also determinants of herbicide use, which is likely to be the case in our framework. For example, one can reasonably suppose that winegrowers who feel concerned about environmental issues are simultaneously more likely to participate in AESs and less likely to use herbicide even in the absence of any AES (they have a low $\text{TFI}^0$). Because it determines the level of $\text{TFI}^0$, the participant group and the comparison group must exhibit similar levels of environmental concern before the AES starts, in order for the comparison we propose to be valid. In the specific context of France, we argue that this assumption is likely to hold.

Indeed, areas under study were eligible to participate in the AES at different dates due to administrative delays, which we believe to be exogenous to winegrowers’ practices. In practice, AES implementation by local operators in priority areas required many administrative procedures that ended up delaying the availability of contracts to farmers. As a first requirement, each priority watershed underwent a hydro-geological diagnostic in order to assess its vulnerability and accurately delineate the limits of the area targeted for the AES. An official decree then had to validate this delimitation. Next, a local operator was nominated to design an agri-environmental project on the basis of a second diagnostic. This second diagnostic aimed to identify the current farming practices in use at the site in order to best adapt the proposed options to the needs of the local farmers. The operator was required to choose from a national menu of options, two options that were to be offered to the farmers at his site for each farming activity. As the last requirement, the final scheme had to be validated by a regional committee that meets once per year. After this process was complete, the operator was then in charge of introducing the AES to the farmers at his site. Farmers willing to participate then had to send in an application form by the 15th of May. The time needed to implement these steps was highly heterogeneous between sites, which contributed to the incremental availability of the AES to farmers in different areas. We make use of this exogenous variation in the timing of AES implementation in order to estimate its causal effect on the quantity of herbicides used by participating farmers.

Because our sample includes only farmers who engaged in the AES as soon as they
were offered the opportunity to do so\textsuperscript{3} we assume that the individuals who entered the scheme in May 2010 and the individuals who entered the scheme in May 2012 had the same probability of entering the scheme in 2010, given $Z$. If we accept the qualitative information available regarding the timing of the program’s implementation, then participants and future-participants differed only in the date they were offered the AES, which is exogenous to their practices. Under this assumption, the impact of the AES in 2011 can then be properly identified through a comparison of the average use of herbicides of participants located on early-approved sites (eligible in 2010) with the average use of herbicides of future participants located on late-approved sites (eligible in 2012).

While we are not able to test the natural experiment assumption directly, we are able to test whether both groups have similar observable characteristics in 2009, i.e. before the first wave of participation in the AES, using data obtained from the French Agricultural Census. In addition, we can test the sensitivity of the identification strategy in order to determine how strongly an unmeasured variable must influence the selection process such that it undermines the effectiveness of the identification strategy. Results of those tests are presented in Section 4.

2.3 Stable Unit Treatment Value Assumption

Finally, even if the natural experiment assumption holds, we must make a second assumption, known as the Stable Unit Treatment Value Assumption (SUTVA) for the identification to be valid (Rubin 1978). SUTVA requires that the treatment received by a subject does not alter the outcome for other subjects. The SUTVA assumption fails if, for example, the quantity of herbicides used by farmers in late approved sites had been influenced by the implementation of the scheme in early approved sites. It is very unlikely that such a phenomenon occurred in our framework, as groups are usually located on sites distant from each other. Participation of first-wave farmers may have encouraged the participation of

\textsuperscript{3}Eligibility and participation indeed coincide for most individuals in the sample. We intentionally drop the very small number of those who did not choose to enter the scheme as soon as it was available because we doubt that those farmers have similar environmental concern and motivation as those who chose to enter without delay.
second-wave farmers to some extent, but we argue that this does not invalidate the identification strategy as long as second-wave participants did not change their practices before they entered the scheme.

### 3 Sample and Data

#### 3.1 Sample

Our sample includes winegrowers located in three counties of the Languedoc-Roussillon region in the south of France - watersheds where water quality improvement has been identified as a priority by public authorities (see Figure 1). Between 2007 and 2013, 414 farmers in this area engaged a total number of 8,672 hectares of land in the AES pertaining to water quality. This amounted to a budget of 8.8 millions €. Winegrowers were given the opportunity to participate in AESs aiming to reduce herbicide use, the main pesticide responsible for nonpoint source water pollution in the area. They were able to choose one or more of four possible options: convert to organic wine growing in exchange for 350 €/ha, eliminate all herbicide use for 243 €/ha, reduce herbicides use by 40% of the regional standard for 141 €/ha, and eliminate herbicide use between vine rows only for 165 €/ha. Of the 4,268 ha of vineyards engaged in the scheme between 2007 and 2012, our database includes 3,390 ha, or approximately 80% of the total area under contract in the region. Table 1 shows that the most frequently chosen option in our sample was eliminating herbicide use between the vine rows only, chosen by 79% of farmers in the engaged areas. This proportion is very close to the actual regional take-up of 72%.

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4 Data are available here: [http://draaf.languedoc-roussillon.agriculture.gouv.fr/Commissions-regionales-agro](http://draaf.languedoc-roussillon.agriculture.gouv.fr/Commissions-regionales-agro)

5 Farmers are eligible to enter the considered AES if: (i) they are located on a priority watershed where the scheme is implemented, (ii) the total annual payment they apply for is at least 100€/ha/year (which corresponds to 0.7 hectares of vines engaged for the option with the lowest payment), (iii) they comply with the farm status requirements.

6 Our data also include an additional 238 hectares engaged in 2013. Unfortunately, we were not able to collect all necessary data regarding the areas under contract in 2013.
3.2 Data

The quantity of herbicides used by winegrowers is private data, and even if most of the winegrowers maintain records of the treatments that they apply, they are under no legal obligation to provide this information. However, data of this sort are routinely solicited of winegrowers participating in AESs. Indeed, every farmer willing to participate to the scheme was required to undergo a diagnosis of his farm by a certified technician who was frequently the site operator himself. As part of this diagnosis, information on the quantity of herbicides used during the previous farming season was collected for each plot on the farm. This appraisal was conducted every year during the entire period of the AES through an annual follow-up. We collected both the initial diagnosis as well as the follow-up documents held by the site operators for almost every farmer participating in the scheme. Our sample includes farmers who entered the scheme in 2010, in 2011, in 2012, or in 2013. For farmers who entered the scheme in 2010 (resp. 2011; 2012; 2013), we were able to collect data on the quantity of herbicides used in 2009, 2010, 2011 and 2012 (resp. 2010, 2011 and 2012; 2011 and 2012; 2012). From these documents, we were able to calculate the quantity of herbicides used by winegrowers on the plots under contract, as measured through the Treatment Frequency Index (TFI). This index represents the number of so-called reference doses of herbicides applied during a farming year (Pingault et al., 2009). The reference dose is often considered the normal dose, as it corresponds to the efficient dose of a product for a specific culture and pest:

\[
\text{TFI} = \frac{\text{treated area}}{\text{total area}} \times \frac{\text{dose used}}{\text{reference dose}}
\]

For example, if the reference dose of an herbicide is spread over the entire area of a plot, then the TFI of the plot equals one. If the herbicide is spread at its reference dose but only under the vine rows, the TFI of the plot equals one third (because the space between vine rows is roughly twice as wide as the vine row).

For purposes of analysis, we matched our TFI data to the French Agricultural Census that was conducted in 2010 by the Department of Statistics of the French Ministry of Agri-
culture. This database contains a detailed description of every farm during the farming year 2009-2010, i.e. before the first wave of participation in the AES in 2011.

### 3.3 Determinants of participation in the AES

The sample used for analysis includes exclusively participants in the AES (Table 2). These winegrowers are expected to differ from other winegrowers in various dimensions. Indeed, many individual-specific factors may determine participation in AESs that target pollution from herbicides. For example, the size of the labor force working on the farm can influence the adoption of alternatives to herbicide use, as these practices are more time-consuming. The availability of water for irrigation can also facilitate weed control through the practice of grassing, as this reduces competition for water in the soil under the vines. In contrast, steep slopes in the vineyard could present an obstacle to implementing mechanized weeding practices. Additionally, the type of grape produced and the wine-making process involved can influence the sale price of the production, and indirectly the motivation and ability of farmers to begin reducing their use of pesticides. Finally, the degree of a household’s reliance on farm income is likely to decrease the probability of participation in an AES, as a reduction in pesticide use is expected to increase yield variability.

In order to study to what extent winegrowers from our sample differ from neighboring non-participants, we estimate a logistic regression where the participation in the AES depends on a variety of individual-specific factors measured in 2009 in the French Agricultural Census. In order for this comparison to make sense, we focus on a subset of winegrowers from this database who were eligible for the AES.\[7\] Results are displayed in Table 3. The results suggest that winegrowers who had received agricultural education or training and whose spouse also works on the farm are two times more likely to participate in the AES. They also indicate that those who produce wine under geographically protected appellations, which guarantees higher sale prices, are more likely to participate in the AES. In our data, the geographical conditions faced by participants do not differ from those faced

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\[7\] As no listing of the eligible farmers was available, we focused on farmers located in the same municipalities as participating farmers, assuming that they were eligible, as well. Indeed, the main criterion for eligibility was the location of the vineyard within one of the areas targeted for water quality recovery.
by non-participants. Interestingly, the proportion of Utilized Agricultural Area (UAA) cultivated without herbicides is not significantly higher among participants, which indicates that winegrowers do not reduce their use of herbicides before they enter the AES.

4 Results

In this section, we present and discuss the results of the estimations based on the natural experiment assumption, as well as those we obtain from various matching estimators. We also present the results of several robustness checks and a sensitivity test. Finally, we present some illustrative figures in order to discuss the likely impact of the AES on water quality.

4.1 Overall impact of the AESs

In order to properly estimate the impact of the AES on farmers, we compare current participants to future participants in the AES. Based on the previous section, we argue that current participants and future participants had similar probabilities to enter the AES in 2010 given their characteristics $Z$. Under this assumption, we can recover the average treatment effect of the AES by comparing them directly without controlling for their characteristics. Our data do not allow us to test whether current participants and future participants had similar characteristics $Z$ before the program starts (such test is called a placebo test). However, we are able to compare means between groups for a large set of observable characteristics measured in 2009, i.e. before the first wave of participation in the AES, using data obtained from the French Agricultural Census. Table 7 displays the mean level of covariates $X$ used in Section 3.3. Columns 1 and 2 refer to current participants in 2011 and future participants respectively. These figures show that the two groups do not differ in most features. This result suggests that there was no selection of a specific type of winegrowers into the first-wave implementation of the AES. Importantly, they did not differ in terms of herbicide use, as the area with zero herbicide use was close to 0.25 in both groups. This result is in line with the natural experiment assumption.
In order to estimate the impact of the AES in 2011, we first compare the TFI between the group of individuals who engaged in the AES in 2010 and the group of those who engaged in 2012. In order to obtain a standard error, we simply regress the outcome on the treatment variable, which is a dummy variable that takes on the value of one when treated and zero otherwise. The results are displayed in Table 4 (Panel A). They show that the AESs have a significant and large overall impact on the TFI in 2011: the quantity of herbicides used by participants in the program was 50% below the quantity that would have been used without the program. We run the same regression excluding the number of farmers who engaged in organic farming schemes because we suspect that they may drive this estimate (Panel B). The result does not change.

We then run the regression on Panel C, in which individuals in the treatment group are those who engaged in 2010 or 2011, while individuals in the control group are those who engaged in 2013. Results show that the quantity of herbicides used by participants in the program in 2012 was 0.45, i.e. 38% below the quantity used in the control group. Unfortunately, this result lacks precision (we reject the null hypothesis at the 10% significance level only). In order to increase the sample size, we test the hypothesis that farmers who engaged in the AES in May 2012 can be considered as controls in 2012. It is indeed reasonable to believe that farmers who engaged in the AES in May 2012 were not able to meet the contractual commitments before the next grape harvest (September 2012) due to the fact that a large portion of herbicide applications occur before May. We thus compare the mean value of the TFI in the subgroup of farmers who engaged in the AES in May 2012 to the mean value of the TFI in the subgroup of farmers who engaged in May 2013. The result shows that the TFI does not differ significantly across these groups. We conclude farmers who engaged in the AES in 2012 can be considered as untreated in 2012. Results of the estimation from the sample in which the individuals used as controls are those who engaged in 2012 or 2013 are displayed in the lower part of Table 4 (Panel D). As expected, the estimate appears to be more precise (we now reject the null hypothesis at the 5% significance level). The impact is now slightly smaller: the quantity of herbicides used by participants

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8 Results of the regression are available from the authors upon request.
9 Compared to Panel C, Panel D includes a larger number of individuals but excludes farmers engaged in
in the program in 2012 was 0.6, i.e. 27% below the quantity used in the control group.

Taken together, the results from Table 4 suggest that the impact of the AESs in 2011 was larger than in 2012. In order to test the assumption that these impacts actually differ, we estimate a panel-data model focusing on the subgroup of individuals for whom we have data on TFI in both years 2011 and 2012:

$$TFI_{it} = \alpha + \beta_0 AES_i + \beta_1 AES_i \times T_t + \beta_2 T_t + \epsilon_{it}$$

where the variable AES takes on the value of one when the farmer is treated and zero otherwise. Individuals used as treated are those who engaged in 2010, and individuals used as controls are those who engaged in 2012. The variable $T$ takes on the value of one in 2011 and zero in 2012, the variable $AES \times T$ is an interactive term, and $\alpha$ refers to the constant term of the model. In this regression model, the estimate of the impact in 2011 equals $\beta_0 + \beta_1$, while the estimate of the impact in 2012 equals $\beta_0$.

Results are displayed in Table 5. Columns (1)-(2) display results from the entire sample. Taking the average effect in both years, our results suggest that the AESs had a significant impact on TFI - specifically a 45% decrease compared to the counterfactual situation (Column 1). Regarding the heterogeneity of the impact across years, results in Column 2 confirm that the impact in 2011 indeed differs from the impact in 2012 (we reject the null hypothesis that $\beta_1$ equals zero at the 1% significance level).

Results in Column 2 moreover show that the effect of “being in 2011”, as measured through the variable $T$, is zero for individuals who participated in the AES ($0.21 - 0.22 \approx 0$), while it is significantly different from zero and positive for individuals who did not participate in the AES ($0.21 \neq 0$). In other words, the mean value of the TFI in the control group was significantly higher in 2011 than in 2012, which explains why the impact in 2011 appears larger than the impact in 2012. This result is in line with rainfall data for these years, which indicates higher weed pressure in 2011 (Direction régionale de l’Environnement, de

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10 As previously, we assume that farmers who engaged in 2012 are considered as untreated in 2012.
11 We use the the random-effects estimator and is a weighted average of the estimates produced by the between and within estimators.
l’Aménagement et du Logement, 2011, 2012). Columns (3)-(4) display results from a sample that excludes farmers engaged in organic farming schemes. The results hold in this case, as well.

4.2 Additional and windfall effects of the “zero herbicide between the rows” option

Next, we turn to the impact analysis of one specific AES, the so-called “zero herbicide between the rows” option, for which the sample size is large enough. Results are displayed in Columns (5)-(6) of Table 5. This AES option is interesting because it is both the most often chosen as well as the least stringent among the measures that target herbicide use - characteristics which are probably related. Results displayed in Column (5) show that the average effect of this AES, taking all years together, is significantly different from zero at the standard level of significance. The TFI in the treated group was only 0.21 points below the TFI in the control group (0.95), which corresponds to a 22% decrease compared to the counterfactual situation. This impact seems small, given that farmers who commit to not applying herbicides between vine rows are expected to have a TFI that equals one third of the counterfactual level.

Results in Column (6) moreover show that the impact of this AES varies across years (we reject the null hypothesis that $\beta_1$ equals zero at the 5% significance level). Specifically, the impact appears significantly different from zero in 2011, but not in 2012. In 2011, the average TFI in the control group was equal to 1.03 (0.84 + 0.19), while the average TFI in the treated group was 0.29 points below (1.03 − 0.29 = 0.74). The story is slightly different in 2012, when the average TFI in the control group reached 0.84 only (which corresponds with lower weed pressure in 2012 due to a drought in the region), while the average TFI in the treated group was only 0.06 points below the counterfactual level (0.84 − 0.06 = 0.78). This result suggests that farmers complied with the contractual commitment during the first year of their agreement (with a TFI close to 0.7) and maintained these practices after-

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12 Public authorities indeed expected that winegrowers would divide the quantity used by 3, as the space between vine rows is roughly twice as wide as the vine row.
wards, while the use of herbicides in the control group fluctuated between the two years (with a TFI close to 1 in 2011 and close to 0.8 in 2012). As the mean TFI in the control group approaches the mean TFI in the treated group in 2012, the impact of the AES on herbicide use becomes statistically null. We can conclude that the impact of this specific AES is all the more weak as weed pressure is low. In extreme cases, like in 2012, participants in the scheme would have use approximately the same quantity of herbicides without any payment. As a consequence, the additional effect of the AES is null and the windfall effect is maximum.

4.3 Discussion on spatial variability

As noted in the theoretical framework, the amount of herbicides farmers apply depends not only on individual-specific factors (such as physical and human capital) but also on area-specific factors pertaining to geographical constraints (e.g. soil type, topography, and climatic conditions like temperature and rainfall). For example, the quantity of herbicides used during rainy years is expected to be higher than during dryer years, all other determinants being equal, because of the higher weed pressure that rainfall generates. Our sample includes farmers who are spread over a fairly wide area. Notably, when comparing average levels of TFI among participants and future participants, to some extent we compare farmers located in the east zone of the region under study to farmers located in the west zone. In order to avoid potential bias in the estimate of the impact of the AES that could arise due to spatial variability in geographical characteristics, we perform the estimations by focusing on a subset of farmers who are very close to each other geographically and who constitute a large enough sample to ensure accurate results. In doing so, we drop 23% of the observations from the initial sample. Results are displayed in Table 6. As in Table 5, columns (1)-(2) display results from the entire sample; columns (3)-(4) display results from a sample that excludes farmers engaged in organic farming schemes; and columns (5)-(6) display results from a sample that includes only farmers engaged in the

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13 Further east, in Gard county, are the territories of Malaven, Camp de Cesar, and Briançon. Farmers in these areas are not separated by more than 40 km.
so-called between-the-rows option. Results appear very similar to those obtained from the initial sample. We conclude that the data we use from western areas do not bias the estimates.

### 4.4 Alternative identification strategy

In relying on the natural experiment assumption, we believe that current participants and future participants did not differ on average in their likelihood to participate in the AES in 2010. Alternatively, if we assume that current participants differ from future participants in some factors \( X \) that are observable to us before the AES starts, we can use a quasi-experimental approach in order to estimate the ATT. The idea behind this is the following: if current participants and future participants are similar on average in all of their characteristics (the natural experiment assumption), then they are similar a fortiori in characteristics that are observable to us. Thus, employing a quasi-experimental approach should provide the same results as the simple comparison of TFI between groups.

The validity of matching estimators of the AES impact in 2011 relies on the following assumption:

\[
E(TFI_0^{2011} | X_{2010}, AES_{2011} = 1) = E(TFI_0^{2011} | X_{2010}, AES_{2011} = 0)
\]

In practice, matching estimators eliminate the selection bias caused by observable characteristics \( X \) by comparing the TFI of current participants with those of observationally-identical matched future-participants [Imbens, 2004]. There are a variety of matching estimators. We use the nearest-neighbor matching estimator [Abadie et al., 2004], the kernel-based matching estimator, and the local linear matching estimator [Leuven and Sianesi, 2003]. The general form of the matching estimators is:

\[
E(TFI^1 - TFI^0 | AES = 1) = \frac{1}{n_1} \sum_{i \in I_1 \cap S_p} (TFI_i^1 - E(TFI_i^0 | AES = 1, X_i))
\]
with

$$E(TFI_i^0 | AES = 1, X_i) = \sum_{j \in I_0} \lambda_{ij} TFI_j^0$$

where $I_1$ denotes the group of treated farmers, $I_0$ denotes the group of untreated farmers, and $n_1$ is the number of treated farmers in $I_1$. $S_P$ denotes the common support, the subset of treated farmers for whom the density of observationally-identical untreated farmers is higher than some cut-off level \cite{Todd2008}. Matching estimators differ in how matched untreated farmers are selected through the matching procedure. This is driven by the weights $\lambda_{ij}$ that we assign to potential matches given their characteristics $X$. The nearest-neighbour matching estimator matches each participating farmer to the one or two closest future-participants (closest in terms of vector $X$).\footnote{We use the asymptotically-consistent estimator of the variance of the nearest-neighbour matching estimator provided by \cite{Abadie2006}, and we implement a bootstrap procedure of 500 repetitions in order to generate an estimator of the variance of the kernel and local linear matching estimators.}

It is important that the covariates $X$ are not affected by the treatment \cite{Imbens2004}, which is why we utilize 2009 values from the French Agricultural Census. Moreover, we also apply the matching procedure to the summary statistic $\Pr(AES_i = 1 | X_i)$, also called the propensity score \cite{Rosenbaum1983}.\footnote{We obtain the individual propensity scores by estimating the probability of participating in the AES, conditional on the control variables $X$, from a sample that includes participants, future-participants and non-participants of the neighbouring areas (see Section\footnote{In this model, the dependent variable takes on the value of one when the individual is a participant or future-participant and zero when the individual never participated in the AES.}3). As expected, given their characteristics, participants and future participants have similar likelihoods of participating in the AES in 2010, as shown in Figure\footnote{In this model, the dependent variable takes on the value of one when the individual is a participant or future-participant and zero when the individual never participated in the AES.}2 and Figure\footnote{In this model, the dependent variable takes on the value of one when the individual is a participant or future-participant and zero when the individual never participated in the AES.}3.}

Another, computationally easier, way to obtain an estimate of the ATT is to run an ordinary least squares regression of the following model:

$$TFI_i = \gamma_0 + \gamma_1 AES_i + \gamma_2 X_i + \mu_i$$

where $\gamma_1$ is the impact that we seek to estimate. However, in addition to the assumption of linearity, doing so would require supposing that $\gamma_1$ is constant across $X$, meaning that the impact of the AES is the same for all participants. Without any evidence for such an asser-
tion, we present the results of the matching approach, which does not require specifying
the functional form of the outcome equation and relaxes the assumption of constant ad-
dditive treatment effects across individuals. We also present the results of linear regressions
as a robustness check.

Table 7 displays the mean level of covariates $X$ from the French Agricultural Census
for farmers in each group. Columns 1 and 2 refer to current participants and future par-
ticipants respectively, who are used for the direct comparison of the mean TFI in 2011.
Column 3 refers to the subset of future participants who ended up in the control group
following the matching procedure. The matching procedure performed quite well in re-
ducing some differences between covariates through the delimitation of the new control
group. Differences that remain statistically significant are not expected to play an impor-
tant role in the decision to use herbicides.

Table 8 gives the estimated ATT in 2011 from the direct comparison between groups
(first row of the table) and from the matching estimators. The results appear remarkably
stable, with a TFI gap between groups of nearly -0.5 points in all cases. We turn next to the
estimated ATT in 2012 (Table 9). Here again our estimates are significant and very similar
to our main result, with a TFI gap between groups close to -0.3 points, although this result
is slightly more scattered and sometimes less precise. These results provide evidence that
our main findings based on the natural experiment assumption do not suffer from any
selection biases that would be due to observable factors.

4.5 Sensitivity test

Rosenbaum (2002) uses an approach that determines how strongly an unmeasured vari-
able must influence the selection process in order to undermine the results of the match-
ing analysis. Two farmers with the same observed characteristics may differ in the odds
of participating in the AES by at most a factor of $\Gamma$. We thus search the critical levels of
$\Gamma$ at which the estimated ATT would become insignificant. This search indicates that the
critical value for $\Gamma$ is greater than 3. This means that two farmers who have the same ob-
servable characteristics $X$ would have to differ in their odds of program participation by a
factor of 3 (200%) in order to render the ATT estimated from the matching procedure insignificant. We can thus conclude that, even though unobservable factors may play a role in the decision to enter the AES, it is very unlikely that they would influence the odds of participation to such a large extent. We are thus confident that our identification strategy performs well.

### 4.6 What is the impact on water quality?

We estimate that the AES leads to a 0.5 point reduction of the TFI in 2011, which means that the participating farmers applied about half the quantity of herbicides that they would have applied in the absence of the scheme. Such an evaluation is an important step toward the assessment of the cost-effectiveness of the AES in improving water quality in French watersheds. Because we do not know the proportion of winegrowers who participate in the scheme in each watershed, the quantity of herbicides used by all non-participants in each watershed, the exact location of each participant, nor the contribution of their lands to global water pollution levels, we are not able to conduct a complete cost-benefit analysis of the studied AES. Nonetheless, we are able to provide some insights regarding the impact of the AES on water quality through some illustrative numbers.

Let us focus on a specific, commonly used herbicide, glyphosate, which has a reference dose of 1,440 g/ha. Depending on soil characteristics, 0.1 to 5% of the applied product ends up in surface or groundwater. This means that at least 1.44 g/ha are likely to be carried away by runoff. An estimated 1,500 cubic meters of water exits each hectare per year as runoff\textsuperscript{16} As a result, assuming that polluted water will replace all clean water in the long term, the concentration of glyphosate in the water would be 0.96 µg/l, which is much higher than the legal limit for drinking water (0.5 µg/l)\textsuperscript{17} Supposing now that all winegrowers in a watershed chose to participate in the AES and that our estimate of the ATT would be the same for all of them, the concentration of glyphosate in the water would

\textsuperscript{16} Runoff can be roughly estimated as the difference between annual rainfalls, approximatively 650 mm in the region under study, and annual evapotranspiration, which is on average 500 mm for vineyards.

\textsuperscript{17} The legal limit for the level of pesticides in drinking water is 0.1 µg/l for each molecule of pesticide, with a maximum cumulated level of 0.5 µg/l for all types of pesticides.
reach only 0.48 µg/l, which is below the legal limit for the level of pesticides in drinking water. Under this scenario, the AES would have reduced the level of herbicides in regional water sources just below the cumulated threshold for drinking water. However, under the assumption that the maximum of 5% of applied herbicides are carried away by runoff, the AES would have lead to a concentration in herbicides in the water exiting the plots equal to 24 µg/l instead of 0.48 µg/l, which is much higher than the legal threshold. These simple calculations suggest that the success of this AES in ensuring water quality is conditional on many factors.

5 Conclusions

The main results of our analysis suggest that the AESs targeting the reduction of herbicide use in French vineyards had a significant impact on participants’ practices. We show that the quantity of herbicides used by participants in the AESs in 2012 was around 30% below what they would have used without the program, and that this impact was significantly higher in 2011 (around 50%). These results are robust to various estimators, robustness checks, and a sensitivity analysis.

We moreover show that variation in the impact of the AES over time can be explained by seasonal differences in weed pressure: while participants comply with the contractual commitment during the first year of their agreement and maintained these practices afterwards, winegrowers in the control group adjust the quantity of herbicides they use according to the weed pressure they face in a given year. The mean value of the TFI in the control group is significantly higher in 2011 (a rainy year) than in 2012 (a dry year). Consequently, the ATT in 2011 appears larger than the ATT in 2012.

Results also indicate that the least demanding but most adopted AES option - “zero herbicide between the vine rows” - has a significant impact on herbicide use in 2011, but no significant impact in 2012. This suggests the presence of windfall effects, which tend to decrease the efficiency of the program by using resources to pay for practices that would have been adopted anyway. This thus suggests that least demanding AES options are ef-
fective in avoiding pollution peaks when weed pressure is high (as in 2011), whereas more demanding AES options guarantee an overall reduction in herbicide use even during easy farming years in which less weed pressure is experienced (as in 2012).

Though additional work is necessary to measure the impact of the scheme on overall water quality within the affected watersheds, our analysis constitutes a first step in this direction by showing that, under certain conditions, these AESs are likely to reduce the level of herbicides in water below the cumulated threshold for drinking water. This result is more likely to hold when the quantity of herbicides carried away by runoff is small.

In conclusion, it is worth-mentioning that all of our estimates rely on existing data. Though these data were available to us, they were based on sometimes sporadic reports submitted by the local site operators themselves. Future AES evaluation would benefit from more complete data, the collection of which could be facilitated by greater oversight of the reporting process on a national level.

References


Table 1: Areas under contract in the sample

<table>
<thead>
<tr>
<th>AES</th>
<th>2010</th>
<th>2011</th>
<th>2012</th>
<th>2013</th>
<th>Total</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>phyto02</td>
<td>92.1</td>
<td>2.9</td>
<td>12.4</td>
<td>41.1</td>
<td>148.6</td>
<td>4%</td>
</tr>
<tr>
<td>phyto04</td>
<td>176.4</td>
<td>3.5</td>
<td>0.0</td>
<td>0.0</td>
<td>179.9</td>
<td>5%</td>
</tr>
<tr>
<td>phyto10</td>
<td>562.9</td>
<td>272.3</td>
<td>1840.8</td>
<td>197.7</td>
<td>2873.7</td>
<td>79%</td>
</tr>
<tr>
<td>couver03</td>
<td>0.0</td>
<td>0.0</td>
<td>17.8</td>
<td>0.0</td>
<td>17.8</td>
<td>0%</td>
</tr>
<tr>
<td>bioconv</td>
<td>119.1</td>
<td>149.2</td>
<td>103.7</td>
<td>0.0</td>
<td>372.0</td>
<td>10%</td>
</tr>
<tr>
<td>biomaint</td>
<td>0.0</td>
<td>0.0</td>
<td>37.7</td>
<td>0.0</td>
<td>37.7</td>
<td>1%</td>
</tr>
<tr>
<td>Total</td>
<td>950.5</td>
<td>427.9</td>
<td>2012.4</td>
<td>238.9</td>
<td>3629.7</td>
<td>100%</td>
</tr>
</tbody>
</table>

Note: Figures are expressed in hectares. AES phyto02 refers to the suppression of herbicide use; AES phyto04 refers to the reduction in herbicides use by 40% of the regional standard; AES phyto10 refers to the suppression of herbicide use between the vine rows; AES bioconv (resp. biomaint) refers to the conversion to (resp. maintaining of) organic wine growing.

Table 2: AES participation in the sample

<table>
<thead>
<tr>
<th>AES</th>
<th>2010</th>
<th>2011</th>
<th>2012</th>
<th>2013</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>phyto02</td>
<td>10</td>
<td>1</td>
<td>2</td>
<td>7</td>
<td>20</td>
</tr>
<tr>
<td>phyto04</td>
<td>6</td>
<td>1</td>
<td>0</td>
<td>0</td>
<td>7</td>
</tr>
<tr>
<td>phyto10</td>
<td>29</td>
<td>11</td>
<td>77</td>
<td>14</td>
<td>131</td>
</tr>
<tr>
<td>couver03</td>
<td>0</td>
<td>0</td>
<td>4</td>
<td>0</td>
<td>4</td>
</tr>
<tr>
<td>bioconv</td>
<td>5</td>
<td>7</td>
<td>7</td>
<td>0</td>
<td>19</td>
</tr>
<tr>
<td>biomaint</td>
<td>0</td>
<td>0</td>
<td>2</td>
<td>0</td>
<td>2</td>
</tr>
<tr>
<td>Total</td>
<td>50</td>
<td>20</td>
<td>92</td>
<td>21</td>
<td>183</td>
</tr>
</tbody>
</table>

Note: AES phyto02 refers to the suppression of herbicide use; AES phyto04 refers to the reduction in herbicides use by 40% of the regional standard; AES phyto10 refers to the suppression of herbicide use between the vine rows; AES bioconv (resp. biomaint) refers to the conversion to (resp. maintaining of) organic wine growing.
<table>
<thead>
<tr>
<th></th>
<th>Odds Ratio</th>
<th>Std. Err</th>
<th>z</th>
<th>P&gt;z</th>
</tr>
</thead>
<tbody>
<tr>
<td>Date of birth</td>
<td>1.03</td>
<td>0.01</td>
<td>2.97</td>
<td>0.00</td>
</tr>
<tr>
<td>Agricultural education: less than baccalaureate (0/1)</td>
<td>2.00</td>
<td>0.48</td>
<td>2.91</td>
<td>0.00</td>
</tr>
<tr>
<td>Agricultural education: more than baccalaureate (0/1)</td>
<td>2.56</td>
<td>0.87</td>
<td>2.75</td>
<td>0.01</td>
</tr>
<tr>
<td>Agricultural training: less than baccalaureate (0/1)</td>
<td>2.27</td>
<td>0.65</td>
<td>2.87</td>
<td>0.00</td>
</tr>
<tr>
<td>Agricultural training: more than baccalaureate (0/1)</td>
<td>2.11</td>
<td>1.21</td>
<td>1.30</td>
<td>0.20</td>
</tr>
<tr>
<td>General education: less than baccalaureate (0/1)</td>
<td>1.31</td>
<td>0.37</td>
<td>0.96</td>
<td>0.34</td>
</tr>
<tr>
<td>General education: more than baccalaureate (0/1)</td>
<td>1.47</td>
<td>0.52</td>
<td>1.09</td>
<td>0.28</td>
</tr>
<tr>
<td>Spouse's main activity: agricultural activity (0/1)</td>
<td>2.38</td>
<td>0.67</td>
<td>3.09</td>
<td>0.00</td>
</tr>
<tr>
<td>Spouse's main activity: non-agricultural activity (0/1)</td>
<td>1.14</td>
<td>0.29</td>
<td>0.52</td>
<td>0.60</td>
</tr>
<tr>
<td>Spouse's main activity: none (0/1)</td>
<td>1.42</td>
<td>0.44</td>
<td>1.12</td>
<td>0.26</td>
</tr>
<tr>
<td>Vineyard surface area (ha)</td>
<td>1.00</td>
<td>0.00</td>
<td>2.82</td>
<td>0.01</td>
</tr>
<tr>
<td>Vineyard surface area (%UAA)</td>
<td>1.85</td>
<td>1.26</td>
<td>0.90</td>
<td>0.37</td>
</tr>
<tr>
<td>Labor (annual work unit)</td>
<td>1.00</td>
<td>0.00</td>
<td>-2.22</td>
<td>0.03</td>
</tr>
<tr>
<td>Production (hl)</td>
<td>1.00</td>
<td>0.00</td>
<td>0.34</td>
<td>0.73</td>
</tr>
<tr>
<td>Surface area without herbicide (%UAA)</td>
<td>0.81</td>
<td>0.23</td>
<td>0.76</td>
<td>0.45</td>
</tr>
<tr>
<td>AOP (%production)</td>
<td>2.90</td>
<td>0.89</td>
<td>3.48</td>
<td>0.00</td>
</tr>
<tr>
<td>Vinification in particular cellar (%production)</td>
<td>0.68</td>
<td>0.18</td>
<td>-1.45</td>
<td>0.15</td>
</tr>
<tr>
<td>Irrigation (%UAA)</td>
<td>0.84</td>
<td>0.91</td>
<td>-0.16</td>
<td>0.87</td>
</tr>
<tr>
<td>Property (%UAA)</td>
<td>0.43</td>
<td>0.12</td>
<td>-3.09</td>
<td>0.00</td>
</tr>
<tr>
<td>Slope (degrees)</td>
<td>1.05</td>
<td>0.06</td>
<td>0.84</td>
<td>0.40</td>
</tr>
</tbody>
</table>

Note: Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis at the 1% (resp. 5%, 10%, 15%) significance level. The sample includes a total number of 1,562 winegrowers: 139 participants in AES and 1,423 winegrowers who were eligible but did not participate in the AES. We created dummy variables for the categories of the variables “Agricultural education”, “Agricultural training”, and “General education” (the reference category is “no baccalaureate”). The reference category of the variable “Spouse’s main activity” is “no spouse”.

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### Table 4: Average treatment effects in 2011 and 2012

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Treated</th>
<th>ATT</th>
<th>s.e.</th>
<th>n0</th>
<th>n1</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>TFI 2011 - Panel A</td>
<td>0.98</td>
<td>0.48</td>
<td>-0.50***</td>
<td>0.09</td>
<td>83</td>
<td>38</td>
<td>121</td>
</tr>
<tr>
<td>TFI 2011 - Panel B</td>
<td>1.04</td>
<td>0.55</td>
<td>-0.49***</td>
<td>0.09</td>
<td>76</td>
<td>31</td>
<td>107</td>
</tr>
<tr>
<td>TFI 2012 - Panel C</td>
<td>0.74</td>
<td>0.45</td>
<td>-0.28*</td>
<td>0.16</td>
<td>15</td>
<td>48</td>
<td>63</td>
</tr>
<tr>
<td>TFI 2012 - Panel D</td>
<td>0.86</td>
<td>0.62</td>
<td>-0.23**</td>
<td>0.09</td>
<td>83</td>
<td>34</td>
<td>117</td>
</tr>
</tbody>
</table>

Note: Column 1 displays the mean value of TFI in the treated group. Column 2 displays the mean value of TFI in the control group. ATT refers to the Average Treatment Effect; s.e. refers to the standard error; n0 (resp. n1) refers to the number of farmers in the control (resp. treated) group. N is the sample size. In Panel A, individuals in the treatment group are those who engaged in 2010 and individuals in the control group are those who engaged in 2012. In Panel B, individuals in the treatment group are those who engaged in 2010 and individuals in the control group are those who engaged in 2012 but farmers engaged in organic farming schemes are excluded. In Panel C, individuals in the treatment group are those who engaged in 2010 or 2011 and individuals in the control group are those who engaged in 2013. In Panel D, individuals in the treatment group are those who engaged in 2010 or 2011 and individuals in the control group are those who engaged in 2012 or 2013. Panel D excludes farmers engaged in organic farming schemes. Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis at the 1% (resp. 5%, 10%, 15%) significance level.

### Table 5: Panel regressions

<table>
<thead>
<tr>
<th></th>
<th>organic included (1)</th>
<th>organic excluded (2)</th>
<th>(3)</th>
<th>(4)</th>
<th>between-the-rows (5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>AES</td>
<td>-0.40***</td>
<td>-0.28***</td>
<td>-0.38***</td>
<td>-0.25***</td>
<td>-0.21**</td>
<td>-0.06</td>
</tr>
<tr>
<td>AES*T</td>
<td>0.08</td>
<td>0.09</td>
<td>0.08</td>
<td>0.09</td>
<td>0.10</td>
<td>0.11</td>
</tr>
<tr>
<td>T</td>
<td>-0.22***</td>
<td>-0.25***</td>
<td>-0.29**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>cons</td>
<td>0.89***</td>
<td>0.77***</td>
<td>0.95***</td>
<td>0.85***</td>
<td>0.95***</td>
<td>0.84***</td>
</tr>
</tbody>
</table>

Note: The variable AES takes on value 1 when the farmer is treated and zero elsewhere; the variable T takes on value 1 in 2011 and zero in 2012; the variable AES*T is an interactive term; cons refers to the constant term of the model. In all regressions individuals used as treated are those who engaged in 2010 and individuals used as controls are those who engaged in 2012. Columns (1)-(2) display results from the whole sample. Columns (3)-(4) display results from a sample which excludes farmers engaged in organic farming schemes. Columns (5)-(6) display results from a sample which includes only farmers engaged in the so-called between-the-rows scheme. Standard errors appear in italics below the coefficient estimates. Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis at the 1% (resp. 5%, 10%, 15%) significance level.
Table 6: Panel regressions - Eastern region only

<table>
<thead>
<tr>
<th></th>
<th>organic included</th>
<th>organic excluded</th>
<th>between-the-row</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>AES</td>
<td>-0.42***</td>
<td>-0.29***</td>
<td>-0.30***</td>
</tr>
<tr>
<td></td>
<td>0.09</td>
<td>0.10</td>
<td>0.09</td>
</tr>
<tr>
<td>AES*2011</td>
<td>-0.23**</td>
<td>-0.29**</td>
<td>-0.26**</td>
</tr>
<tr>
<td></td>
<td>0.10</td>
<td>0.11</td>
<td>0.13</td>
</tr>
<tr>
<td>2011</td>
<td>0.21***</td>
<td>0.19***</td>
<td>0.19***</td>
</tr>
<tr>
<td></td>
<td>0.05</td>
<td>0.05</td>
<td>0.05</td>
</tr>
<tr>
<td>cons</td>
<td>0.93***</td>
<td>0.81***</td>
<td>0.97***</td>
</tr>
<tr>
<td></td>
<td>0.04</td>
<td>0.05</td>
<td>0.04</td>
</tr>
<tr>
<td>Nb. Obs.</td>
<td>193</td>
<td>193</td>
<td>170</td>
</tr>
<tr>
<td>Nb. Farmers</td>
<td>100</td>
<td>100</td>
<td>88</td>
</tr>
</tbody>
</table>

Note: The variable AES takes on value 1 when the farmer is treated and zero elsewhere; the variable T takes on value 1 in 2011 and zero in 2012; the variable AES*T is an interactive term; cons refers to the constant term of the model. In all regressions individuals used as treated are those who engaged in 2010 and individuals used as controls are those who engaged in 2012. Columns (1)-(2) display results from the whole sample. Columns (3)-(4) display results from a sample which excludes farmers engaged in organic farming schemes. Columns (5)-(6) display results from a sample which includes only farmers engaged in the so-called between-the-rows scheme. Standard errors appear in italics below the coefficient estimates. Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis at the 1% (resp. 5%, 10%, 15%) significance level.
Table 7: Balancing tests on pre-treatment variables

<table>
<thead>
<tr>
<th>Variables</th>
<th>(1) treated</th>
<th>(2) untreated</th>
<th>(3) matched</th>
<th>(4) stat</th>
<th>(5) stat</th>
</tr>
</thead>
<tbody>
<tr>
<td>Date of birth</td>
<td>1966</td>
<td>1964</td>
<td>1.39</td>
<td>1963</td>
<td>3.12 ***</td>
</tr>
<tr>
<td>Agricultural education: less than baccalaureate</td>
<td>0.67</td>
<td>0.35</td>
<td>9.89 ***</td>
<td>0.53</td>
<td>7.90 **</td>
</tr>
<tr>
<td>Agricultural education: more than baccalaureate</td>
<td>0.17</td>
<td>0.26</td>
<td>1.20</td>
<td>0.17</td>
<td>4.00</td>
</tr>
<tr>
<td>Agricultural training: less than baccalaureate</td>
<td>0.06</td>
<td>0.16</td>
<td>2.27</td>
<td>0.19</td>
<td>0.35</td>
</tr>
<tr>
<td>Agricultural training: more than baccalaureate</td>
<td>0.03</td>
<td>0.04</td>
<td>0.09</td>
<td>0.03</td>
<td>0.00</td>
</tr>
<tr>
<td>General education: less than baccalaureate</td>
<td>0.53</td>
<td>0.71</td>
<td>3.78 *</td>
<td>0.61</td>
<td>11.76 ***</td>
</tr>
<tr>
<td>General education: more than baccalaureate</td>
<td>0.25</td>
<td>0.16</td>
<td>1.44</td>
<td>0.13</td>
<td>5.14 *</td>
</tr>
<tr>
<td>Spouse’s main activity: agricultural activity</td>
<td>0.42</td>
<td>0.17</td>
<td>8.08 ***</td>
<td>0.19</td>
<td>21.78 ***</td>
</tr>
<tr>
<td>Spouse’s main activity: non-agricultural activity</td>
<td>0.17</td>
<td>0.30</td>
<td>2.24</td>
<td>0.20</td>
<td>4.47</td>
</tr>
<tr>
<td>Spouse’s main activity: none</td>
<td>0.06</td>
<td>0.18</td>
<td>3.22 *</td>
<td>0.03</td>
<td>1.01</td>
</tr>
<tr>
<td>Vineyard surface area (ha)</td>
<td>2811</td>
<td>3221</td>
<td>0.98</td>
<td>2592</td>
<td>0.94</td>
</tr>
<tr>
<td>Vineyard surface area (%UAA)</td>
<td>0.91</td>
<td>0.90</td>
<td>0.70</td>
<td>0.93</td>
<td>-0.87</td>
</tr>
<tr>
<td>Labor (annual work unit)</td>
<td>2332</td>
<td>2774</td>
<td>0.91</td>
<td>2103</td>
<td>0.94</td>
</tr>
<tr>
<td>Production (hl)</td>
<td>1103</td>
<td>1246</td>
<td>0.66</td>
<td>941</td>
<td>1.42</td>
</tr>
<tr>
<td>Surface area without herbicide (%UAA)</td>
<td>0.29</td>
<td>0.21</td>
<td>1.00</td>
<td>0.22</td>
<td>1.57</td>
</tr>
<tr>
<td>AOP (%production)</td>
<td>0.76</td>
<td>0.81</td>
<td>0.89</td>
<td>0.83</td>
<td>-1.84 *</td>
</tr>
<tr>
<td>Vinification in particular cellar (%production)</td>
<td>0.43</td>
<td>0.22</td>
<td>-2.30 **</td>
<td>0.22</td>
<td>2.71 **</td>
</tr>
<tr>
<td>Irrigation (%UAA)</td>
<td>0.02</td>
<td>0.03</td>
<td>0.54</td>
<td>0.01</td>
<td>1.11</td>
</tr>
<tr>
<td>Property (%UAA)</td>
<td>0.38</td>
<td>0.32</td>
<td>0.75</td>
<td>0.40</td>
<td>-0.32</td>
</tr>
<tr>
<td>Slope (degrees)</td>
<td>4.28</td>
<td>3.34</td>
<td>2.67 ***</td>
<td>3.47</td>
<td>1.84 *</td>
</tr>
</tbody>
</table>

Note: **stat** is the statistics of the test that tests the null hypothesis that the means for both groups are equal (t-test for continuous variables and chi-squared test for categorical variables). Two asterisks ** (resp. *) denote rejection of the null hypothesis at the 1% (resp. 5%) significance level. The sample includes 37 treated who engaged in 2010 and 84 untreated who engaged in 2012 (see Panel A from Table 4). We created dummy variables for the categories of the variables “Agricultural education”, “Agricultural training”, and “General education” (the reference category is “no baccalaureate”). The reference category of the variable “Spouse’s main activity” is “no spouse”.
Table 8: Treatment effects in 2011 - Matching estimators

<table>
<thead>
<tr>
<th></th>
<th>ATT</th>
<th>s.e.</th>
<th>stat</th>
</tr>
</thead>
<tbody>
<tr>
<td>OLS</td>
<td>-0.53</td>
<td>0.09</td>
<td>-5.74 ***</td>
</tr>
<tr>
<td>OLS (X)</td>
<td>-0.52</td>
<td>0.11</td>
<td>-4.86 ***</td>
</tr>
<tr>
<td>OLS (pscore)</td>
<td>-0.53</td>
<td>0.09</td>
<td>-5.80 ***</td>
</tr>
<tr>
<td>One Nearest Neighbour (X)</td>
<td>-0.46</td>
<td>0.13</td>
<td>-3.58 ***</td>
</tr>
<tr>
<td>One Nearest Neighbour (pscore)</td>
<td>-0.53</td>
<td>0.13</td>
<td>-3.93 ***</td>
</tr>
<tr>
<td>Two Nearest Neighbour (X)</td>
<td>-0.49</td>
<td>0.12</td>
<td>-4.00 ***</td>
</tr>
<tr>
<td>Two Nearest Neighbour (pscore)</td>
<td>-0.54</td>
<td>0.10</td>
<td>-5.24 ***</td>
</tr>
<tr>
<td>Kernel regression</td>
<td>-0.52</td>
<td>0.09</td>
<td>-5.51 ***</td>
</tr>
<tr>
<td>Local linear regression</td>
<td>-0.47</td>
<td>0.14</td>
<td>-3.49 ***</td>
</tr>
</tbody>
</table>

Note: ATT refers to the Average Treatment Effect; s.e. refers to the standard error; stat refers to the test statistic. Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis (ATT = 0) at the 1% (resp. 5%, 10%, 15%) significance level. We use the asymptotically-consistent estimator of the variance of the nearest-neighbor matching estimator and we implement a bootstrap procedure (500 repetitions) to get an estimator of the variance of the kernel and the local linear matching estimators. The sample size is 113, which is smaller than sample size in Table 4. This is because some participants in the AES have not been found in the French Agricultural Census.

Table 9: Treatment effects in 2012 - Matching estimators

<table>
<thead>
<tr>
<th></th>
<th>ATT</th>
<th>s.e.</th>
<th>stat</th>
</tr>
</thead>
<tbody>
<tr>
<td>OLS</td>
<td>-0.23</td>
<td>0.10</td>
<td>-2.45 **</td>
</tr>
<tr>
<td>OLS (X)</td>
<td>-0.29</td>
<td>0.12</td>
<td>-2.38 **</td>
</tr>
<tr>
<td>OLS (pscore)</td>
<td>-0.22</td>
<td>0.10</td>
<td>-2.29 **</td>
</tr>
<tr>
<td>One Nearest Neighbour (X)</td>
<td>-0.22</td>
<td>0.11</td>
<td>-1.88 *</td>
</tr>
<tr>
<td>One Nearest Neighbour (pscore)</td>
<td>-0.18</td>
<td>0.11</td>
<td>-1.59 °</td>
</tr>
<tr>
<td>Two Nearest Neighbour (X)</td>
<td>-0.29</td>
<td>0.14</td>
<td>-2.10 **</td>
</tr>
<tr>
<td>Two Nearest Neighbour (pscore)</td>
<td>-0.19</td>
<td>0.10</td>
<td>-1.87 *</td>
</tr>
<tr>
<td>Kernel regression</td>
<td>-0.22</td>
<td>0.09</td>
<td>-2.40 **</td>
</tr>
<tr>
<td>Local linear regression</td>
<td>-0.17</td>
<td>0.13</td>
<td>-1.29</td>
</tr>
</tbody>
</table>

Note: ATT refers to the Average Treatment Effect; s.e. refers to the standard error; stat refers to the test statistic. Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis (ATT = 0) at the 1% (resp. 5%, 10%, 15%) significance level. We use the asymptotically-consistent estimator of the variance of the nearest-neighbor matching estimator and we implement a bootstrap procedure (500 repetitions) to get an estimator of the variance of the kernel and the local linear matching estimators. The sample size is 108, which is smaller than sample size in Table 4. This is because some participants in the AES have not been found in the French Agricultural Census.
Figure 1: Location of surveyed farmers

Figure 2: Density of propensity scores by group - ATT in 2011
Figure 3: Density of propensity scores by group - ATT in 2012
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